which the curves severally belong, and with the origins of co-ordinates of the curves situated in a straight line perpendicular to their planes, and with the axes of co-ordinates of all of them parallel in pairs to one another, and then the curved surface is to be formed so as to pass through those curved lines smoothly or evenly.[*] The curved surface so obtained exhibits in a very obvious way the remarkable phenomena of the voluminal conditions at and near the critical point of temperature and pressure, in comparison with the voluminal conditions throughout other parts of the range of gradually varying temperatures and pressures to which it extends, and even throughout a far wider range into which it can in imagination be conceived to be extended. It helps to afford a clear view of the nature and meaning of the continuity of the liquid and gaseous states of matter. It does so by its own obvious continuity throughout its expanse round the end of the range of points of pressure and temperature where an abrupt change of volume can occur by boiling or condensing. On the curved surface in the model Dr. Andrews' curves for the temperatures 13°·1, 21°·5, 31°·1, 32°·5, 35°·5 and 48°·1 Centigrade, which afford the data for its construction, may with advantage be all shown drawn in their proper places. The model admits of easily exhibiting in due relation to one another a second set of curves, in which each would be for a constant pressure, and in each of which the co-ordinates would represent temperatures and corresponding volumes. It may be used in various ways for affording quantitative relations interpolated among those more immediately given by the experiments.

We may now, aided by the conception of this model, return to the consideration of continuity or discontinuity in the curves in crossing the boiling stage. Let us suppose an indefinite number of curves, each for one constant temperature, to be drawn on the model, the several temperatures differing in succession by very small intervals, and the curves consequently being sections of the curved surface by numerous planes closely spaced parallel to one another and to the plane containing the pair of co-ordinate axes for pressure and volume. Now we can see that, as we pass from curve to curve in approaching towards the critical point from the higher temperatures, the tangent to the curve at the steepest point or point of inflection is rotating, so that its inclination to the plane of the co-ordinate axes for pressure and temperature, which we may regard as horizontal, increases till, at the critical point, it becomes a right angle. Then it appears very natural to suppose that in proceeding onwards past the critical point, to curves successively for lower and lower temperatures, the tangent at the point of inflection would continue its rotation, and the angle of its inclination, which before was acute, would now become obtuse. It seems much more natural to make such a supposition as this than to suppose that in passing the critical point from higher into lower temperatures the curved line, or the curved surface to which it belongs, should break itself asunder, and should come to have a part of its conceivable continuous course absolutely deficient. It thus seems natural to suppose that in some sense there is continuity in each of the successive curves by courses such as those drawn in the accompanying diagram as dotted curves uniting continuously the curves for the ordinary gaseous state with those for the ordinary liquid state.

The physical conditions corresponding to the extension of the curve from *a* to some point *b* we have seen are perfectly attainable in practice. Some extension of the gaseous curve into points of temperature and pressure below what I have called the boiling, or condensing line, as for instance some extension such as from *c* to *d* in the figure, I think we need not despair of practically realising in physical operations. As a likely mode in which to bring steam continuing gaseous to points of pressure and temperature at which it would collapse to liquid water if it had any particle of liquid water present along with it, or if other circumstances were present capable of affording some apparently *requisite conditions for enabling it to make a beginning of the change of state*,[†] I would suggest the admitting speedily of dry steam nearly at its condensing temperature for its pressure (or, to use a common expression, *nearly saturated*) into a vessel with a piston or plunger, all kept hotter than the steam, and then allowing the steam to expand till by its expansion it would be cooled below its condensing point for its pressure; and yet I would suppose that if this were done with very careful precautions the steam might not condense, on account of the cooled steam being surrounded entirely with a thin film of superheated steam close to the superheated containing vessel. The fact of its not condensing might perhaps best be ascertained by observations on its volume and pressure. Such an experiment as that sketched out here would not be easily made, and unless it were conducted with very great precautions, there could be no reasonable expectation of success in its attempt; and perhaps it might not be possible so completely to avoid the presence of dust or other dense particles in the steam as to make it prove successful. I mention it, however, as appearing to be founded on correct principles, and as tending to suggest desirable courses for experimental researches. The overhanging part of the curve from *e* to *f* seems to represent a state in which there would be some kind of unstable equilibrium; and so, although the curve there appears to have some important theoretical significance, yet the states represented by its various points would be unattainable throughout any ordinary mass of the fluid. It seems to represent conditions of co-existent temperature, pressure, and volume, in which, if all parts of a mass of fluid were placed, it would be in equilibrium, but out of which it would be led to rush, partly into the rarer state of gas, and partly into the denser state of liquid, by the slightest inequality of temperature or of density in any part relatively to other parts. I might proceed to state, in support of these views, several considerations founded on the ordinary statical theory of capillary or superficial phenomena of liquids, which is dependent on the supposition of an attraction acting very intensely for very small distances, and causing intense pressure in liquids over and above the pressure applied by the containing vessel and measureable by any pressure-gauge. That statical theory has fitted remarkably well to many observed phenomena, and has sometimes even led to the forecasting of new results in advance of experiment. Hence, although dynamic or kinetic theories of the constitution and pressure of fluids now seem likely to supersede any statical theory, yet phenomena may still be discussed according to the principles of statical theory; and there may be considerable likelihood that conditions explained or rendered probable under the statical theory would have some corresponding explanation or confirmation under any true theory by which the statical might come to be superseded. With a view to brevity, however, and to the avoidance of putting forward speculations perhaps partly rash, though, I think, not devoid of real significance, I shall not at present enter on details of these considerations, but shall leave them with merely the slight suggestion now offered, and with the suggestion mentioned in an earlier part of the present paper, of the question whether in an extremely thin lamina of gradual transition from a liquid to its own gas, at their visible face of demarcation, conditions may not exist in a stable state having a correspondence with the unstable conditions here theoretically conceived.

ALTERNATION OF GENERATIONS IN FUNGI

It has long since been shown that certain fungi pass through an alternation of generations on the same plant. The Rev. M. J. Berkeley demonstrated that in the case of the common "bunt" at least four consecutive forms of reproductive cellules were produced. In the majority of Uredines there are two forms of fruit, but these can scarcely be regarded as an alternation of generations, since there is no evidence that the spores of *Trichobasis* by germination, or otherwise, produce the bilocular spores of *Puccinia*. In *Podisoma* and *Gymnosporangium* (if the two genera are really distinct) the bilocular spores germinate freely and produce unilocular secondary spores. Prof.

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[*] For the practical execution of this, it is well to commence with a rectangular block of wood, and then carefully to pare it down, applying, from time to time, the various curves as templets to it; and proceeding according to the general methods followed in a shipbuilder's modelling room in cutting out small models of ships according to curves laid down on paper as cross sections of the required model at various places in its length.

[†] The principle that "the particles of a substance, when existing all in one state only, and in continuous contact with one another, or in contact only under special circumstances with other substances, experience *a difficulty of making a beginning of their change of state*, whether from liquid to solid, or from liquid to gaseous, or probably also from solid to liquid," was proposed by me, and, so far as I am aware, was first announced in a paper by me in the Proceedings of the Royal Society for November 24, 1859, and in a paper submitted to the British Association in the same year. In the present paper, at the place to which this note is annexed, I adduce the like further supposition that *a difficulty of making a beginning of change of state* from gaseous to liquid may also probably exist.

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Oersted contends that if these secondary unilocular spores are sown upon young plants of the *Sorbus aucuparia*, they will germinate, and that the ends of the germinating filaments penetrating the tissues of the leaf of the sorb will in turn produce the spermagonia and peridia of *Rœstelia cornuta*. This is very similar to the deductions of Prof. de Bary that the spores of the *Æcidium* which flourishes on the berberry may be employed to inoculate young plants of wheat, and will produce as a result the wheat-mildew (*Puccinia graminis*), which he contends is another generation of the berberry fungus completed upon a different host. (*See* NATURE, vol. ii. p. 318)

Such experiments as those of Professors Oersted and De Bary must always prove unsatisfactory unless performed with extraordinary care, and until confirmed by other observers. One or two strong presumptions can always be urged against them, and require to be boldly faced. Wheat is very subject to the attacks of mildew (*Puccinia*), and the results claimed for certain experiments are that they have produced by inoculation with other spores the common *Puccinia* upon wheat plants, to which the wheat is particularly addicted all the world over. Admitting that the *Æcidium* spores sown on the leaves of young wheat plants germinate, and that the germinating filaments enter the tissues of the leaf, are we therefore justified in affirming, or admitting that the inoculating spores produce the *Puccinia* which ultimately exhibits itself? Is it not more feasible to believe that the germination of the foreign spores have only served to stimulate the latent germs of the *Puccinia* already present in the tissues of the wheat plant? What guarantee is afforded by those who have already experimented, that the wheat plants experimented upon would not ultimately, without inoculation, have developed precisely the same parasite as that supposed to have been produced by inoculation? Assuming also that the experiment was pursued in the opposite direction, and that the spores of the wheat mildew were sown upon young plants of the berberry, if the *Æcidium* should soon afterwards appear on the leaves, it is easy enough to jump to the conclusion that they were produced by inoculation, but assumption is insufficient since the berberry is very subject, year after year, to bear on some of its leaves the peridia of the *Æcidium*. What evidence could be given that the *Æcidium* would never have appeared but for the inoculation? It is manifest that no amount of care in cultivation under bell glasses or other exclusion from foreign influences is sufficient against a contingency which dates back to the seed of the nurse-plant.

If the sowing of the spores of *Æcidium* upon the leaves of wheat resulted in the production of an *Æcidium* identical with it, or if the inoculation of berberry with wheat mildew was succeeded by the development of a *Puccinia* of a very similar character, it would not be so difficult to believe in both cases that the resulting forms might have been caused by inoculation. When the fungi assumed to be produced by inoculation are those to which the nurse-plants are particularly and specially subject, the evidence should be very strong before it is affirmed that a very natural phenomenon had an unnatural* cause.

The evolution of *Rœstelia* on the leaves of the "mountain ash" by inoculation with *Podisoma* spores is quite analogous to the berberry and wheat fungi. It is common enough to find the *Podisoma* on junipers, and the *Rœstelia* on "mountain ash," and the presumption would be, if young plants of "mountain ash" were covered up ever so carefully with bell glasses, notwithstanding that the leaves had been sprinkled with the spores of a dozen other species of fungi, if *Rœstelia* made its appearance, that it bore no relation whatever to any of the foreign spores which had been sown upon it, but would have been there independent of inoculation, or bell glasses, or a dozen like contingencies.

In both cases to which allusion has been made above, there is need of the strongest evidence to show that the ultimate parasite would not have made its appearance but for the inoculation, or that the whole chain was completed which connected the inoculating spore with the parasite produced. It would be folly to contend against facts for the sake of theory, and absurd to combat conclusions fairly deduced from ascertained facts; but in this instance we are bound to contend, in honesty to our convictions, that in neither case has Oersted or De Bary shown to our satisfaction that they were justified in declaring for an alternation of generations of fungi in which the stages were passed on different nurse-plants. When the facts are confirmed and established will be time enough to inquire whether both stages are essential the one to the other, and, if so, how it is that mildewed wheat in such great profusion can be found in districts where berberry bushes are unknown, or why the *Rœstelia* on the leaves of pear trees should be so common in counties where scarcely a savin can be found.

I have been led to these observations partly because some writers have accepted the conclusions at once as if they were incontrovertible facts, and partly because I have personally been charged with ignoring (by silence, it is presumed) the results of De Bary and Oersted's experiments, whereas I only claim the privilege of doubting where I would not dare to deny.

M. C. COOKE

THE SCIENCE AND ART DEPARTMENT

THE following important Minute on the subject of Science instruction has recently been issued by the Committee of the Privy Council on Education:—

It appears desirable that the instruction of students in Science, after they have completed the course of the ordinary elementary school, should be carried on more methodically than is at present the case, and that they should not attempt to grapple with the more advanced forms of Science until they have received sound and practical instruction in those subjects which constitute the groundwork of all the physical sciences.

To this end the course of instruction specified below has been prepared as adapted both to secondary day schools and to night classes.

It will depend on circumstances, especially if the student can only attend night classes, how many subjects he can take up in one year. It must therefore be understood that the course should not only comprise the subjects named below, but also that they should be taken in the order in which they are stated.

The terminology used is that of the Science and Art Directories. The syllabus of subjects there given states precisely what is included under each head. And it is assumed that before commencing the following course, the student will have been made acquainted, in the elementary school, with the elements of arithmetic, and the primary conceptions of physical science.

COURSE OF INSTRUCTION.—*First Year.*—Mathematics (Subject V., First Stage); Freehand Drawing (2nd Grade Art); Practical Plane Geometry (2nd Grade Art); Elementary Mechanics, including the physical properties of liquids and gases (Subject VI., First Stage); Physics: Acoustics, Light and Heat (Subject VIII., First Stage). *Second Year.*—Chemistry, Inorganic (Subject X., First Stage), with practical work; Physics: Magnetism and Electricity, frictional and voltaic (Subject IX., First Stage); Mathematics (Second Stage and, if possible, Fourth Stage, Subject V.); Practical Geometry, Plane and Solid (Subject I., First Stage); Animal Physiology, if possible (Subject XIV., First Stage). The student

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* The term "unnatural" is employed here in the sense that the presumed cause is one of which we have no experience, and which is contrary to the ordinary course of nature.

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